THE JOURNAL OF PHILOSOPHY

VOL. LV, NO. 17: AUGUST 14, 1958

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PUBLISHED FORTNIGHTLY

BY THE

JOURNAL OF PHILOSOPHY, INC.

THE JOURNAL OF PHILOSOPHY

FOUNDED BY FREDERICK J. E. WOODBRIDGE AND WENDELL T. BUSH

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Communications to the editors should be addressed to the Managing Editor, 713 Philosophy Hall, Columbia University, New York 27, N. Y. Subscriptions and advertisements should be sent to THE JOURNAL OF PHILOSOPHY, 713 Philosophy Hall, Columbia University, New York 27, N. Y. The annual subscription price (twenty-six numbers) is $5.25. The price of single numbers is 35 cents, double numbers, 60 cents.

THE JOURNAL OF PHILOSOPHY is owned by the Journal of Philosophy, Inc., and published fortnightly at Prince and Lemon Sts., Lancaster, Pa. Editorial Office: 713 Philosophy Hall, Columbia University, New York 27, N. Y. President, Albert G. Redpath; Vice President, John H. Randall, Jr.; Treasurer, Frederick A. Dewey; Secretary, Corliss Lamont.

Entered as second class matter, March 18, 1921, at the Post Office at Lancaster, Pa., under Act of Congress of March 3, 1879.

Acceptance for mailing at special rate of postage provided for in paragraph (d-2), section 34.40, P. L. & R. of 1948, authorized March 11, 1952.

PUBLICATIONS

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VOLUME LV, No. 17 AUGUST 14, 1958

THE JOURNAL OF PHILOSOPHY

THE RIDDLE OF INDUCTION*

I BELIEVE there is something wrong with "ordinary language" replies to the critic of induction and, after criticizing these replies, suggest some alternative ways of answering him.

I

It is indeed difficult to criticize "ordinary language" analysts in a sweeping fashion since they are highly individualistic and use various techniques to achieve similar ends. Nevertheless, it seems to me that these analysts, in their work on the problem of induction, invariably fail in one of two ways: either they do not eliminate the critic's demand or, if they do, they use notions of 'meaning,' either implicitly or explicitly, which are no more acceptable than the views about induction which they would eliminate.[1]

Consider, first, the ubiquitous contextual argument: there *is* no problem of justifying induction in general, only particular problems of justifying particular inductions.[2] It is sensible to talk about justifying particular scientific tests and techniques, since there are significant alternatives among which a choice must be made. However, it is queer to talk about "justifying" induction since there is no significant alternative. Clairvoyance, soothsaying, and oracular pronouncement are not *possible*, albeit not scientifically accredited, ways of non-inductively finding out about the future, as one might think, since these methods are vitiated if unsuccessful and are warranted by simple enumerative induction if successful. Again, since there is no significant alternative to induction as a way of finding out the future, as there are significant alternatives to a particular inductive procedure, it is pointless to talk about the justification of induction while it is not at all pointless to talk about the justification of a particular inductive procedure.

* Read, in part, at the American Philosophical Association meeting, Harvard University, December 27–29, 1957.

[1] I am not including in this generalization, of course, those analysts who confine themselves to an empirical study of language in use without trying thereby to nullify "philosophical" assertions.

[2] Cf. P. F. Strawson, *Introduction to Logical Theory*, pp. 256–263.

One might deny the correctness of this contextual argument by insisting there *are* possible non-inductive ways of knowing the future. The only grounds an outsider would ever have for believing another person to be clairvoyant or possessed of prevision would be, indeed, the success of these procedures. The clairvoyant or "previewer," however, after he became convinced he was one, through induction, perhaps, would have an explanation of his very success; namely, that he directly apprehends the future and so avoids the perils of the inductive leap. And one must admit, despite its many shortcomings, that the notion of celestial omniscience is not self-inconsistent; and granting this much, it would seem more than queer to talk about checking this prevision inductively. More to the point, however, one might allow the correctness of the contextual argument but insist that it leaves the critic unscathed. The contextual argument proves only that induction itself cannot be justified in the way a particular induction can; it does not prove that induction itself cannot be justified in any way. But since the critic is not demanding for induction itself the kind of justification it is possible to give particular inductions but demands a logically conclusive justification of inductive inference—i.e., a demonstration that inductive evidence entails the inductive conclusion,—he is not touched by the contextual argument. Now the demand for a deductive justification may well be an unreasonable one, but the contextual argument does not show that it is. This type of argument, therefore, must be accompanied, as it usually is, by some meaning analysis in terms of which this demand is shown to be unreasonable.

The *ignoratio elenchi* argument, it seems to me, has a similar weakness. According to this argument,[3] the critic of induction does the unremarkable thing of refuting the view that positive observation, or stratification, etc., is a good reason for accepting an inductive conclusion when 'good reason' means 'logically conclusive reason.' But, of course, he certainly has not refuted the claim that positive observations do constitute a good reason for accepting an inductive conclusion where 'good reason' is used in its ordinary sense. The *ignoratio elenchi* which the critic of induction commits, because he is heedless of his redefinition, consists of refuting the former claim when allegedly he is refuting the latter one. I do not wish to argue, of course, whether or not critics of induction ever do make this mistake but I do wish to insist that if they do they are completely off the track of what, in their explicit moments, they have to say or, at any rate, what they need to say. The critic

[3] Cf. Paul Edwards, "Bertrand Russell's Doubts About Induction," *Logic and Language* (First Series), ed. by Anthony Flew, pp. 55–79.

of induction willingly concedes that in the ordinary sense of 'good reason' there are perfectly good reasons for accepting inductive inferences as valid but argues—for reasons by now quite familiar—that the ordinary sense of 'good reason' is an insufficient sense of 'good reason' for justifying inductive inference. The sense of 'good reason' sufficient for this justification, one more "fundamental" than the ordinary sense, is, to be sure, 'logically conclusive reason.' Now, again, this demand for a logically conclusive reason may well be an unreasonable one, but the *ignoratio elenchi* argument does not show that it is. This type of argument, therefore, must be supplemented by some meaning analysis which shows the unreasonableness of the demand.[4]

"Ordinary language" analysts, both implicitly and explicitly, have committed themselves to numerous views of 'meaning' which supposedly show the meaningless, self-contradictory, or incorrect nature of the critic's demand. Notice, first, the words that the critic of induction characteristically uses: How can you know that you have a *truly representative sample*? In Peirce's case of the boatload of wheat, even though the wheat is stirred and stirred, how can you know that there is not some latent pocket of unstirred wheat which would destroy the validity of the calculation? But if you can never know the sample is *really fair*, what justification do you have for calling inductive inference knowledge at all? One retort might be: The terms 'fair sample' and 'representative sample' in 'real fair sample' and 'true representative sample' in this argument occur either in a self-contradictory or vacuous way. 'Fair sample,' in its ordinary usage, is defined in terms of its capacity to satisfy the requirements of careful "stirring," stratification, or whatever else can be done along these lines. 'Whatever else can be done along these lines,' of course, keeps the definition open so it can include future refinements and new techniques which have arisen and no doubt will continue to arise in science and everyday life. In fact, however, to say that 'fair sample,' in the sense in which it is ordinarily used, is defined by these requirements does not imply that the requirements are definite and precise. Nevertheless, for some "restricted purposes"—apparently for refuting the critic!—the term might be introduced into discourse by a formal explanation of its meaning—in which case 'fair sample' is *defined* in terms of its capacity to satisfy the specified vague requirements. Now, to return to the critic's argument, if

[4] There are, of course, other techniques to unravel the "confusions" which lead to "paradoxical" assertions which could, with a little ingenuity, be applied to our problem, but similar criticisms, I suspect, would apply to them.

he uses 'fair sample' initially in the way people commonly do (which he apparently does, since we understood him at the outset), he is referring to a kind of sample revealed by one of the vague requirements, or some refinement thereof. If he does utilize this meaning, but also talks about a 'real fair sample' which one can never know he has or have good reason to believe he has no matter how many or how refined are the requirements it meets, then his use of the term is self-contradictory, meaning that at once it both does and does not satisfy the ordinary requirements of a fair sample. If the use of the term is to avoid being self-contradictory, it must function in some new sense. Since no new sense is provided, however, if it is not self-contradictory it is vacuous. (Sometimes the concept 'use' is involved in this type of argument,[5] but I shall attend later to this notion.)

The decisive difficulty with this maneuver, it seems to me, is this: By identifying the meanings of 'fair sample' and 'sample that is stirred, stratified, etc.,' a person is unable to give as a reason why a given sample is a fair one the fact that it has been stirred, stratified, etc.; but this, of course, is precisely what one wants to be able to do. I cannot give as a reason why something is what it is, a synonymous phrase. I have given no support to my statement 'John is my brother' by saying, "John is my male-sibling." And if one asks, "Why is this a fair sample?" or "Is this a fair sample?" I have not answered him at all if, having asserted the equivalence of 'fair sample' and 'sample that is stirred, . . . ,' I reply, "Because it has been stirred, stratified, etc." All I would have managed to say, making the proper substitutions, is, "This sample is fair because it is a fair sample."

Another retort to the critic of induction might be: Since the critic's claim that the ordinary sense of 'good reason' is insufficient to justify inductive inference and the justifier's claim that a "uniformity of nature" principle is required are not "factual" or "empirical" in nature, they must be construed as statements about language, albeit the critic and justifier mistakenly believe that they are not talking about language at all. The critic and justifier, impressed by the incorrigibility of logical and mathematical reasoning, can only be interpreted as wanting (or proposing or recommending) to use 'good reason' and 'warranted inference' only in reference to these paradigms, thereby abandoning the ordinary senses in which these words are used. Then, in order to refute them, one shows it is unnecessary or incorrect to abandon the ordi-

[5] Cf. Max Black, *Language and Philosophy*, p. 13. Cf. also pp. 3–22, 65–68.

nary uses (the ordinary distinctions will still have to be made all over again in the new terminology, etc.). Let me add that this type of analysis, although it has, of course, been used as stated,[6] is not monolithic (indeed, none of them are). Every part of it (e.g., 'Philosophical assertions are really about language') is still used separately and in conjunction with other concepts already examined and to be examined;[7] but my criticisms, as far as I can see, accompany them wherever they go.

I have kept 'factual' and 'empirical' in double quotes because it is not always clear what the proponents of this type of analysis mean when they say philosophical assertions are not factual or empirical.[8] Sometimes they apparently mean that philosophical assertions are not factual because the same facts are available to philosophical disputants who nevertheless reach opposite conclusions. Moreover, no further fact could possibly resolve their dispute. The truth or falsity of "philosophical" statements, in short, cannot, even theoretically, be determined by recourse to "actual" or "possible" facts. This claim, I take it, amounts to saying that "philosophical" assertions, since they do not make any difference in either actual or possible experience, are not verifiable, and what is not verifiable cannot be factual. But, of course, one who says that nature is uniform is insisting that although this assertion is not a matter of experience it is a factual assumption about the universe which one must make in order to account for things which are experienced. He would, in short, reject the present analysis because it amounts to an (unguarded) equivalent of the verifiability theory and so exhibits the well-known deficiencies in consistency and adequacy. Analysts, to be sure, sometimes interpret 'Philosophical assertions are not factual' differently; e.g., 'Sceptical assertions, with their redefinitions of ordinary terms, are *necessary* propositions.' However, assume one can show 'Ordinary evidence is insufficient to justify inductive inference' is a necessary proposition (since, for the critic, 'sufficient evidence' means 'logically conclusive evidence'), does it follow that the critic's assertion is "about language," a proposal or whatever, albeit he mistakenly thinks it is about something else? Not at all, since the analysts (or "elucidators") are unable to offer any criterion for deciding when a sentence ostensibly about something else is "really" about

[6] For analysis and criticism of this technique, cf. Roderick M. Chisholm, "Comments on the 'Proposal Theory' of Philosophy," this JOURNAL, Vol. XLIX (1952), pp. 301–306.

[7] Sometimes, in current analyses, 'x is not factual' appears as 'x has no test procedures for its application.'

[8] Chisholm, *op. cit.*, pp. 302 ff.

language (as paradoxical a claim as any paradox it would help eliminate). And they cannot operate on the assumption that any sentence *must* be about language if it is not empirical, for this operating principle would be, simply, another variant of the verifiability theory, different, certainly, in numerous ways from the positivistic version but identical with it in eliminating the possibility of unverifiable factual assertions. Moreover, one cannot *refute* linguistic proposals or recommendations, or show they are incorrect;[9] all one can do is to offer reasons for not accepting them. But the proponents of the proposals will offer counter reasons for *accepting* them with the result that all the old philosophical assertions and disputes will occur in a new context, and, as we have seen, numerous ideal languages will flourish as "competitors" of ordinary language—justifiably for anything this argument shows to the contrary.

The most ubiquitous notion of meaning, and one which can be applied in numerous ways to the critic of induction, concerns the concept 'use.' Unfortunately the use of 'use' is often far from explicit.[10] Sometimes analysts, when they say of an expression that it has no use, apparently mean that it is *meaningless* in one or more of the senses already analyzed, or in one or more parts of these senses; or that it has no "test procedures" which can be specified for its application; or that, like 'unextended material body,' it is self-contradictory; or that, irrelevantly, a philosophical redefinition of an ordinary word has no application to ordinary situations—and, conversely, of course, that it has a use if it is meaningful in one or more of the senses already analyzed, etc., or has "test procedures," or is unlike 'unextended material body,' or has an application to ordinary situations. Sometimes analysts, when they say of an expression that it has a use, mean that there are typical situations to which anyone who understands a certain descriptive expression would be prepared to apply it unhesitatingly. The expression acquires its meaning from its regular application to, and its meaning is usually taught by reference to, these typical situations (paradigm cases). That an expression has a use in this sense implies, of course, that it has a denotation; consequently the critic's claim that 'fair sample' can never be known to be exemplified is necessarily incorrect or mistaken since one can show that 'fair sample' is an expression that *does* have a use in this sense.

[9] Cf. Morris Lazerowitz, "Moore's Paradox," in *The Philosophy of G. E. Moore* (Library of Living Philosophers), p. 376.

[10] Cf. J. W. N. Watkins, "Farewell to the Paradigm-Case Argument," *Analysis*, Vol. 18, No. 2 (1957), p. 28.

(The recent controversy between Flew and Watkins centers around the application of this type of argument, used in many contexts, to the concept 'acting of his own free-will.'[11])

(i) References like 'the meaning of an expression is usually taught' or 'can be taught' cannot play any significant role in this analysis since the critic of induction has at his disposal many plausible hypotheses about ways in which certain concepts are learned, even though on his view there are no genuine instances of these concepts. (ii) If the regular application of 'fair sample' to paradigm cases of careful "stirring," stratification, etc., ostensively *defines* it, then careful "stirring," stratification, etc., cannot be used, as we have seen, as reasons why a sample is fair. (The same difficulty, of course, can be worked out with 'free-will.') (iii) No reason is given why philosophical concepts like 'fair sample' and 'free-will' cannot be considered compound descriptive terms and so not necessarily ostensively definable.[12] (iv) When the critic says that 'fair sample' can never be known to be exemplified, or that there is never any good reason for accepting an inductive inference, he uses the terms 'fair sample' and 'good reason' in his extended or unusual senses. However, since the present refutation depends on interpreting him as using these concepts *correctly* (i.e., in their ordinary senses) and denying that these ordinary senses are ever exemplified, it misses its mark.

Moreover, most ordinary language analysts themselves, while agreeing that the over-all strategy of this type refutation is sound, would insist that this refutation is *inadequate*. One never does enough simply to point out *that* an expression has a use but must explicate *what* this use is in order to avoid the application of the expression to *prima facie* but not genuine exemplifications of it. Flew, I take it, in his reply to Watkins,[13] has realized just this deficiency in his earlier arguments. He writes that examples used in explaining the use of a word must satisfy a certain "general specification," which, in fact, distinguishes between *prima facie* and genuine paradigm cases. It is simply not enough, e.g., to say 'free-will' is exemplified when a man marries without any social pressure or duress; in view of psychoanalytic knowledge it is necessary to add, say, that he did what he did after rejecting possible alternative courses of action, etc. Again, it simply would not be enough to say 'fair sample' is exemplified by any sample the elements of which have been selected randomly; in view of well-known inductive prob-

[11] *Analysis*, Vol. 18, No. 2 (1957), pp. 25–42.

[12] Cf. Watkins, *ibid.*, p. 29.

[13] *Analysis*, Vol. 18, No. 2 (1957), pp. 34–40.

lems, it is necessary to add that the population and sample be stratified, etc. However, whenever one has (adequately!) specified a *paradigm*, then one proceeds, on Flew's account, essentially as before. One shows of a specific case that it fits, *as a matter of fact*, the specification of the paradigm, and thus just *is*, tautologically, an instance of 'fair sample,' 'free-will,' or whatever the paradigm is that is specified. And since the critic, when he says that one never acts of free-will or never has good reason for accepting an inductive inference, is denying this tautology he is caught up in a self-contradiction. Flew's reformulation, however, while it avoids criticism (iii), *still does not avoid (iv)*. And it does not avoid (ii) either, since this criticism, phrased in the context of ostensive definition, can be rephrased in the context of "general specification" of 'fair sample.'

Finally, analysts would insist that elucidation of the use of a term, in addition to the assertion that it has a use, is also insufficient. The analyst, in addition, must discover, and by clarification remove, the misleading analogies and arguments which lead philosophers to make their "paradoxical" assertions.[14] At this point, however, casuistic analysis has gone full circle, since these techniques are usually of the contextual, *ignoratio elenchi*, and presuppositional types. There are, of course, other "ordinary language" techniques which I have not examined at all—and, no doubt, there are more to come—but all too frequently they turn out to be, on counter-analysis, simply variations on the examined themes.

II

If one denies 'It is possible to justify induction' is meaningless or self-contradictory he does not need to believe, on the other hand, as if it were the only alternative, that he must meet the critic's demand for a justification of induction. It does not follow that because a demand is meaningful it is also significant and requires to be met. If one shows that a demand is meaningless or self-contradictory then he has *eliminated* it and precludes anyone from making it. On the other hand, if one shows that a demand is pointless or trivial then he has *attenuated* it and so may dissuade others from making it or, at any rate, persuade himself that he can safely ignore it. Now any demand which could not in principle ever be met because any answer to it is bound to fail, or, if not bound to fail, doomed to prove too much, etc., while not self-contradictory is *pointless* (not trivial), and while it cannot be

[14] Norman Malcolm, "Philosophy for Philosophers," *Philosophical Review*, Vol. 60 (1951), p. 340.

eliminated it can be attenuated (by pointing out these deficiencies) and thus safely ignored. However, can one show that the critic's demand for a conclusive defense of induction (whether his intent be remedial or sceptical) cannot in principle ever be met since any answer to it is bound to fail, or must prove too much, and thus attenuate the demand?

The first attenuation has long been with us. A principle of the uniformity of nature type or limited variety type is not a logical principle since its denial is not self-contradictory—and, indeed, it must be contingent if it is to do the job of justification required of it; but, then, of course, *it* requires some inductive grounds for acceptance and so the original difficulty inevitably reappears and a conclusive defense inevitably fails. Or, in terms of 'reasonableness': "The overwhelming objection to the assimilation of all induction to deduction is that this would require that we should reasonably believe a very general empirical major premiss, the reasonableness of belief in which would have to be justified by another inductive argument."[15] To be sure, this type of principle might be said to have an initial probability in the sense of subjective expectancy—in which case, however, one has not got beyond Hume's analysis in terms of custom or habit. And any objective concept of probability, whether degree of confirmation or statistical, "would presuppose a principle of induction by means of which we could ascertain the probability of such world hypotheses in comparison with the (infinite) range of their alternatives."[16] But cannot 'degree of confirmation' assimilate induction to deduction without reference to any principle of this type?[17] Apparently not, since "a deductive system of probability statements will only give the probability of an inductive hypothesis on the basis of its confirmation in a set of instances (its 'posterior probability') as an arithmetical product one of whose factors is its probability before it has been confronted with experience (its 'prior probability'); and a supreme major premiss is necessary to insure that this prior probability should be greater than zero."[18] (I shall return to 'degree of confirmation' in a subsequent context.)

Recently some philosophers,[19] trying to avoid these difficulties

[15] R. B. Braithwaite, *Scientific Explanation*, p. 259.

[16] Herbert Feigl, "De Principiis Non Disputandum. . . ?," *Philosophical Analysis*, ed. by Max Black, p. 136.

[17] Cf. John W. Lenz, "Carnap on Defining 'Degree of Confirmation,'" *Philosophy of Science*, Vol. 23 (1956), pp. 230–236.

[18] Braithwaite, *op. cit.*, p. 259.

[19] A. C. Ewing, e.g., in *The Fundamental Questions of Philosophy*, Chapter VIII.

and salvage a deductive justification of induction, have said that the entailment view of causality yields the deductive grounds of inductive inference. A term is defined, in part, by its causal relations; so if "*a*" turned out, in the future, not to exhibit the required and expected causal relations, it would be necessary only to say it was not therefore a genuine instance of *a*. This stratagem, however, successfully insures only that if one knows he has a genuine instance of *a*, then he knows that *x* must necessarily follow in the future, or simply, in the unobserved; but unfortunately it simply shifts the critic's doubts from the inductive inference to the empirical question of whether or not one really has a genuine instance of *a*. Some philosophers, too, still take Kant's principle of causality, conceived as a *pre-condition of knowledge*, as a justification of induction: the human understanding itself impresses the causal order upon the data of the senses. Unfortunately, however, if this principle is to do the job required of it, it would have to include the assumption that human understanding can be relied upon to continue without fail to impress its forms on future data. And, in any case, one could not be certain for any specific effect that he really had the correct cause; and so he could never know certainly that any extrapolation is justified.[20] But, in a Kantian vein, could we not call a major premise of the uniformity or limited variety type a *pre-condition* of knowledge—a principle which is neither directly knowable nor inductively or deductively related to propositions directly knowable but a principle which has to be *postulated*, or assumed to be true, in order to justify other knowledge or reasonable belief? However, this view does not avoid the difficulty inherent in any deductive justification of induction since, on this view, the principle is still a contingent one and "to call an empirical generalization a *postulate* . . . does not make it any the less an empirical generalization; nor does renaming a proposition provide a new method for getting to know it."[21]

There are, of course, other ways to attenuate the critic's demand. E.g., it is pointless to make the demand since, even if it could be met, an answer either presupposes confidence in careful inductive procedures or else justifies any inductive inference whatever. Assume, in the case of the boatload of wheat, that one gives the cargo a few "quick turns," takes samples, and hits upon a ratio. No one would suggest that we justify the extrapolation of this ratio by including a uniformity principle, or whatever, among the

[20] Cf. G. H. von Wright, *The Logical Problem of Induction* (Second Revised Edition), pp. 27 ff.

[21] Braithwaite, *op. cit.*, p. 259, footnote.

premises, since the ratio would likely be upset by further more careful and controlled investigation. However, after giving the cargo one hundred "good turns," taking samples, and hitting upon a ratio, then one might suggest that we justify the extrapolation of this ratio by including a uniformity premise, since the ratio is less likely to be upset by further investigation. In this case, however, one reason, at least, in the conclusive justification of any given inference is confidence in careful inductive procedures. And if one denies completely any confidence in careful inductive procedures, then he must necessarily invoke the uniformity principle in any inductive inference whatever, the one resulting from a few quick turns as well as the one resulting from the careful hundred (since he no longer has any reason for discriminating between them), and so he justifies any inductive inference whatever—the trap, in fact, always waiting for philosophers, of proving or justifying too much rather than too little.

The critic of induction, of course, could reply: Even if you attenuate my demand, *you* must produce some reason for using or adopting inductive procedures since you admit 'It is possible to justify induction' is not meaningless or self-contradictory and have allowed indirectly, by arguing against the contextualist, that there is some general problem of defending induction. I accede to this new demand (for a *justificatio actionis*) although I would insist that answering it amounts to an attenuation of *this* demand, since the answers are so thoroughly simple and obvious. (The obviousness of the answers reflects the *triviality* of the demand.) To ask a person, in all seriousness, what reason he has for using or adopting inductive procedures is like asking a miner with a going claim why he continues to work it. The answer is extremely simple: Gold makes getting through life easier and less hazardous. To be sure, I don't know the claim will last indefinitely; indeed I may be on a short "run of luck," but, of course, the only way to discover that is to keep mining and in the meantime getting the gold and getting through life easier! And, certainly, if the gold gives out I'll drop working the claim. So it is with induction: Induction makes getting through life easier and less hazardous. To be sure, we don't know that it will always continue to be a successful guide; indeed we may be on a short run of luck (or the universe may suffer a breakdown—Peirce in reverse), but, of course, the only way to discover that is to keep on using inductive procedures and in the meantime making inductive inferences and getting through life easier. And, certainly, if inductive procedures give out I'll stop using them (and give out too!). There are other reasons, of

course, for using inductive procedures—including the sort of thing one would say to a child who was unconvinced that he would get burned *next* time—but they are all equally simple and obvious and so would continue the attenuation of the demand. And one must not run the psychological risk of marshaling many answers since this might suggest the demand is difficult to meet.

It might appear plausible to interpret the current "vindications" of induction as attenuations of the demand for a *justificatio actionis*. Some writers,[22] e.g., call their vindications "simple tautologies" or "trivial" as if to attenuate the demand for them, but that they do not intend them so appears from additional comments like "But nevertheless they are illuminating" or "They are necessary for the sake of philosophical clarification." Notice the vindicator's reply to this criticism, "Vindications, even if correct, prove so little"—namely, "A weak defense of the use of inductive procedures is better than nothing at all" or "Philosophers do not seem grateful for small mercies." If, however, the question is simply, Why use inductive procedures?, the correct answer would be, "Because there is so little to prove!" The authors of "means-ends" vindications, however, believe that they can prove something, albeit little, about the *success* of inductive procedures, namely, that if any way of knowing the future works induction does, or if there is any order in nature induction will reveal it. However, these vindications which "concern the choice of means for the attainment of an end" have serious deficiencies. Consider this vindication. The end desired in the use of inductive procedures is "successful prediction, [or] more generally, true conclusions of nondemonstrative inference"[23]; and the question is whether these means are adequate to achieve this end. While one cannot prove they *must* be successful, he can prove that if there is an order in nature not too deeply hidden, then a consistent application of the rule of induction will reveal it—and this proof vindicates the use of the rule. More specifically, Feigl writes that Carnap has shown "there is a large class, in fact a continuum, of inductive rules (or what is tantamount: of definitions of degree of confirmation), of which Reichenbach's rule is an element, which all share the following significant feature: If the world has some degree of order at all, predictions

[22] Herbert Feigl, "Philosophy of Science of Logical Empiricism," *Minnesota Studies in the Philosophy of Science*, Vol. I, p. 30; "De Principiis . . . ," *loc. cit.*, p. 138; "Validation and Vindication: An Analysis of the Nature and Limits of Ethical Arguments," *Readings in Ethical Theory*, ed. by W. Sellars and J. Hospers, p. 675.

[23] Feigl, "De Principiis . . . ," *loc. cit.*, p. 136.

made according to any one of the inductive rules will in the long run (strictly speaking, in the limit) not only converge with the others, but can also be shown (deductively!) to be the only type of predictions that utilize evidence methodically and are capable of anticipating that order of nature." [24]

There is, however, a crucial difficulty in Carnap's method of vindication. The fundamental problem in Carnap's logical theory of probability is how one can choose an adequate *c*-function. Questions of economy and aesthetic satisfaction aside, an adequate *c*-function is one in which the degree of confirmation of hypotheses corresponds with the actual truth frequency of these hypotheses. "The more clearly these two values, one a logically ascertained value, the other an empirically ascertained value, correspond, the more successful as a guide for predictions will the *c*-function be. The more closely these two values correspond the better will be the performance of the *c*-function." [25] However, it should be recalled, if one chooses for future use the *c*-function which has met this requirement most closely in the past (say Carnap's c^*-function rather than the straight rule or the Keynes-Wittgenstein *c*-function), then he is assuming that this function will continue to be successful in the future. But at this point, of course, the whole problem of induction, Feigl's assertion notwithstanding, reasserts itself. Moreover, as Lenz remarks, "Carnap is right when he calls the adoption of a certain *c*-function a practical decision which is not true or false. The objection is that the decision is based upon a synthetic hypothesis which is true or false but whose truth value is not known. Calling the choice of a *c*-function a practical decision in no way avoids the above difficulty." [26]

Certain "self-corrective" vindications,[27] which again concern the success of induction in meeting ends and only *prima facie* resemble part of my simple argument, also have certain difficulties. According to this argument, sampling enables us to make inferences about future experience; but, when that experience comes, if it does not conform to the inference it is at once seen to constitute part of the enlarged corrected data of sampling, since the inductive method must avail itself of every kind of experience. Thus the inference, which was only provisional, corrects itself at last or in the long run. "At last," in cases like the boatload of wheat, unfortunately, can only be when the need for any inference has passed because there

[24] Feigl, *Minnesota Studies* . . . , p. 29.
[25] Lenz, *loc. cit.*, p. 233.
[26] *Ibid.*, p. 234.
[27] Cf. Black, *Problems of Analysis*, pp. 169, 170–171.

is nothing left to infer to. That is to say, the more nearly certain we become of the accuracy of our ratio, the less use we have for it, for the more nearly exhaustive is our examination of the class. On the other hand, when the class is inexhaustible, then the long run is an indefinitely "long run" and so there is no corrective "at last."[28]

Not all vindications apparently consist in judgments about means and ends, although it is difficult to decide the question since the relationships among the various vindications are frequently obscure and sometimes conflict.[29] Moreover, the 'validation,' the correlate of 'vindication,' is not without difficulty.[30] Validating principles—i.e., locutions which determine whole systems of justification (as distinct from vindication)—are construed to be stipulations, definitions, or conventions; but these principles, since they allegedly provide grounds or reasons for other assertions, could hardly be definitions or conventional rules of substitution. (Cf. my criticism of the first ordinary language meaning analysis.) They must, in short, have some categorial status to do the job required of them.

There are, needless to say, types of demands and answers in the complex riddle of induction which we have not even mentioned let alone analyzed,[31] some of which need to be met in different ways while others, I suspect, can be met by pointing out difficulties similar to the ones examined.

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SELF-SUPPORTING INDUCTIVE ARGUMENTS

THE use of inductive rules has often led to true conclusions about matters of fact. Common sense regards this as a good reason for trusting inductive rules in the future, if due precautions are taken against error. Yet an argument from success in the past to probable success in the future itself uses an inductive rule, and therefore seems circular. Nothing would be accomplished by any argument that needed to assume the reliability of an inductive rule in order to establish that rule's reliability.

[28] 'Limit,' 'practical limit,' etc., I believe, defer but do not avoid the difficulty.

[29] Cf. Black, *Problems of Analysis*, pp. 167–168, and Feigl, *Minnesota Studies . . .*, p. 30.

[30] Cf. footnote 22.

[31] E.g., self-supporting and probability justifications.

Suppose that inferences governed by some inductive rule have usually resulted in true conclusions; and let an inference from this fact to the probable reliability of the rule in the future be called a *second-order* inference. So long as the rule by which the second-order inference is governed differs from the rule whose reliability is to be affirmed, there will be no appearance of circularity. But if the second-order inference is governed by the very same rule of inference whose reliability is affirmed in the conclusion, the vicious circularity seems blatant.

Must we, then, reject forthwith every second-order inductive argument purporting to support the very rule of inference by which the argument itself is governed? Contrary to general opinion, a plausible case can be made for saying, No.[1] Properly constructed and interpreted, such "self-supporting" inferences, as I shall continue to call them, can satisfy all the conditions for legitimate inductive inference: when an inductive rule has been reliable (has generated true conclusions from true premises more often than not) in the past, a second-order inductive inference governed by the same rule can show that the rule deserves to be trusted in its next application.

The reasons I have given for this contention have recently been sharply criticized by Professor Wesley C. Salmon.[2] In trying to answer his precisely worded objections, I hope to make clearer the view I have been defending and to dispel some lingering misapprehensions.

My original example of a legitimate self-supporting inductive argument was the following:[3]

(a) In most instances of the use of R in arguments with true premises examined in a wide variety of conditions, R has been successful.

Hence (probably):

In the next instance to be encountered of the use of R in an argument with a true premise, R will be successful.

[1] See Max Black, *Problems of Analysis* (Ithaca, N. Y., 1954), chapter 11, and R. B. Braithwaite, *Scientific Explanation* (Cambridge, 1953), chapter 8.

[2] See Wesley C. Salmon, "Should We Attempt to Justify Induction?" *Philosophical Studies*, Vol. VIII, No. 3 (April, 1957), pp. 45–47.

[3] See *Problems*, page 197, where the argument is called '(a_2)' and the rule by which it is governed 'R_2.' At that place, I also presented another self-supporting argument with a more sweeping conclusion of the *general* reliability of the corresponding rule. But since I was unable to accept the premise of that argument, or the reliability of the rule it employed, I shall follow Salmon in discussing only the argument presented above.

The rule of inductive inference mentioned in the premise and the conclusion of the above argument is:

> R: To argue from *Most instances of A's examined in a wide variety of conditions have been B* to (probably) *The next A to be encountered will be B.*

Thus the second-order argument (a) uses the rule *R* in showing that the same rule will be "successful" (will generate a true conclusion from a true premise [4]) in the next encountered instance of its use.

The rule, *R*, stated above is not intended to be a "supreme rule" of induction, from which all other inductive rules can be derived; nor is it claimed that *R*, as it stands, is a wholly acceptable rule for inductive inference. The unsolved problem of a satisfactory formulation of canons of inductive inference will arise only incidentally in the present discussion. The rule *R* and the associated argument (a) are to serve merely to illustrate the logical problems that arise in connection with self-supporting arguments: the considerations to be adduced in defense of (a) could be adapted to fit many other self-supporting arguments.

The proposed exculpation of the self-supporting argument (a) from the charge of vicious circularity is linked to a feature of the corresponding rule *R* that must be carefully noted. Inductive arguments governed by *R* vary in "strength"[5] according to the number and variety of the favorable instances reported in the premise. So, although *R* permits us to assert a certain conclusion categorically, it is to be understood throughout that the strength of the assertion fluctuates with the character of the evidence. If only a small number of instances have been examined and the relative frequency of favorable instances (*A*'s that are *B*) is little better than a half, the strength of the argument may be close to zero; while a vast predominance of favorable instances in a very large sample of observations justifies a conclusion affirmed with nearly maximal strength. The presence of the word "probably" in the original formulation of *R* indicates the variability of strength of the corresponding argument; in more refined substitutes for *R*, pro-

[4] Here and throughout this discussion, I assume for simplicity that all the premises of any argument or inference considered have been conjoined into a single statement.

[5] In *Problems*, page 193, I spoke, with the same intention, of the "degree of support" given to the conclusion by the premise. If the latter has the form *m/n A's examined in a wide variety of conditions have been B*, it is natural to suppose that the strength of the argument increases as *m* increases, and also as *m/n* increases. A plausible formula for the "strength" of the argument might be $(1 - e^{-m})(2m/n - 1)$.

vision might be made for some precise measure of the associated degree of strength.

Variability in strength is an important respect in which inductive arguments differ sharply from deductive ones. If a deductive argument is not valid, it must be *in*valid, no intermediate cases being conceivable; but a legitimate inductive argument, whose conclusion may properly be affirmed on the evidence supplied, may still be very weak. Appraisal of an inductive argument admits of degrees.

Similar remarks apply to inductive rules, as contrasted with deductive ones. A deductive rule is either valid or invalid—*tertium non datur*; but at any time in the history of the employment of an inductive rule, it has what may be called a *degree of reliability* depending upon its ratio of successes in previous applications. A legitimate or correct inductive rule may still be a weak one: appraisal of an inductive rule admits of degrees.

Now in claiming that the second-order argument (a) *supports* the rule R, I am claiming that the argument raises the degree of reliability of the rule, and hence the strength of the arguments in which it will be used; I have no intention of claiming that the self-supporting argument can definitively establish or demonstrate that the rule is correct. Indeed, I do not know what an outright demonstration of the correctness or legitimacy of an inductive rule would be like. My attempted rebuttal of Salmon's objections will turn upon the possibility of raising the degree of reliability of an inductive rule, as already explained.

The contribution made by the second-order argument (a) to strengthening the rule R by which it is governed can be made plain by a hypothetical illustration. Suppose evidence is available that $\frac{4}{5}$ of the A's so far examined have been B, and it is proposed, by an application of the rule R, to draw the inference that the next A to be encountered will be B. For the sake of simplicity the proposed argument may be taken to have a strength of $\frac{4}{5}$.[6] Before accepting the conclusion about the next A, we may wish to consider the available evidence about past successes of the rule R. Suppose, for the sake of argument, that we know R to have been successful in $\frac{9}{10}$ of the cases in which it has been previously used. If so, the second-order argument affirms with strength $\frac{9}{10}$ that R will be successful in the next instance of its use. But the "next instance" is before us, as the argument whose premise is that $\frac{4}{5}$ of the A's have been B. For R to be "successful" in this instance

[6] This means taking m/n as the measure of strength, rather than some more complicated formula like the one suggested in footnote 5 above. The argument does not depend upon the exact form of the measure of strength.

is for the conclusion of the first-order argument to be true; the strength of the second-order argument is therefore immediately transferred to the first-order argument. Before invoking the second-order argument, we were entitled to affirm the conclusion of the first-order argument with a strength of no better than $4/5$, but we are now able to raise the strength to $9/10$. Conversely, if the second-order argument had shown R to have been unsuccessful in less than $4/5$ of its previous uses, our confidence in the proposed conclusion of the first-order argument would have been diminished.

There is no mystery about the transfer of strength from the second-order argument to the first-order argument: the evidence cited in the former amplifies the evidence immediately relevant to the latter. Evidence concerning the proportion of A's found to have been B permits the direct inference, with strength $4/5$, that the next A to be encountered will be B. It is, however, permissible to view the situation in another aspect as concerned with the extrapolation of an already observed statistical association between true premises of a certain sort and a corresponding conclusion. The evidence takes the form: In 9 cases out of 10, the truth of a statement of the form *m/n X's have been found to be Y's* has been found associated in a wide variety of cases with the truth of the statement *The next X to be encountered was Y.*[7] This is better evidence than that cited in the premise of the original first-order argument: it is therefore to be expected that the strength of the conclusion shall be raised.

It should be noticed that the evidence cited in the second-order argument is not merely greater in amount than the evidence cited in the first-order argument. If R has been successfully used for drawing conclusions about fish, neutrons, planets, etc. (the "wide variety of conditions" mentioned in the premise of the second-order argument), it would be illegitimate to coalesce such heterogeneous kinds of objects into a single class for the sake of a more extensive *first-order* argument. Proceeding to 'second-order' considerations allows us to combine the results of previous inductive inquiries in a way which would not otherwise be possible.

Nothing in this conception of inductive method requires us to remain satisfied with the second-order argument. If circumstances warrant, and suitable evidence can be found, we might be led to formulate third- or even higher-order arguments. These might conceivably result in lowering the measures of strength we at present attach to certain arguments in which R is used. But if this were to happen, we would not have been shown to have been mistaken

[7] We might wish to restrict the second-order argument to cases in which the ratio m/n was close to $4/5$. Other refinements readily suggest themselves.

in previously attaching these measures of strength. Nor is it required that a first-order argument be checked against a corresponding second-order argument before the former can properly be used. If we have no reason to think that *R* is unsuccessful most of the time, or is objectionable on some logical grounds, that is enough to make our employment of it so far reasonable. The function of higher-order arguments in the tangled web of inductive method is to permit us to progress from relatively imprecise and uncritical methods to methods whose degrees of reliability and limits of applicability have themselves been checked by inductive investigations. It is in this way that inductive method becomes self-regulating and, if all goes well, self-supporting.

Salmon's objections to the foregoing conception are summarized by him as follows:

> The so-called self-supporting arguments are . . . circular in the following precise sense: the conclusiveness of the argument cannot be established without assuming the truth of the conclusion. It happens, in this case, that the assumption of the truth of the conclusion is required to establish the correctness of the rules of inference used rather than the truth of the premises, but that makes the argument no less viciously circular. The circularity lies in regarding the facts stated in the premises as *evidence* for the conclusion, rather than as evidence against the conclusion or as no evidence either positive or negative. To regard the facts in the premises as evidence for the conclusion is to assume that the rule of inference used in the argument is a correct one. And this is precisely what is to be proved. If the conclusion is denied, then the facts stated in the premises are no longer evidence for the conclusion.[8]

Comments: (1) Salmon's reference to "conclusiveness" smacks too much of the appraisal of deductive argument. An inductive argument is not required to be "conclusive" if that means that its conclusion is entailed or logically implied by its premises; it is, of course, required to be correct or legitimate, but that means only that the rule of inductive inference shall be reliable—shall usually lead from true premises to true conclusions. The correctness of an inductive argument could only depend upon the truth of its conclusion if the latter asserted the reliability of the rule by which the argument was governed. But this was not the case in our argument (a). The conclusion there was that *R* would be successful in the next instance of its use: this might very well prove to be false without impugning the reliability of *R*. Salmon was plainly mistaken if he thought that the falsity of (a)'s conclusion entails the incorrectness of the rule by which (a) is governed.[9]

[8] Salmon, *loc. cit.*, p. 47.

[9] I conjecture that Salmon was led into making this mistake by forgetting the conclusion of the argument that he correctly reproduces at the foot of page 45 of his article. It is a sheer blunder to say "A given inductive rule can be

(2) Can the *correctness* of argument (a) be "established without assuming the truth of the conclusion" of (a)? Well, if "established" means the same as "proved by a deductive argument," the answer must be that the correctness of (a) cannot be established at all. But again, a correct inductive argument in support of the rule governing (a) can certainly be constructed without assuming (a)'s conclusion. We do not have to assume that R will be successful in the next instance in order to argue correctly that the available evidence supports the reliability of R.

(3) Salmon says: "To regard the facts in the premises as evidence for the conclusion is to assume that the rule of inference used in the argument is a correct one." In using the rule of inference we certainly *treat* it as correct: we would not use it if we had good reasons for suspecting it to be unreliable. If this is what Salmon means, what he says is right, but not damaging to the correctness of (a). But he would be plainly wrong if he maintained that an assertion of the correctness of (a) was an additional premise required by (a), or that an argument to the effect that (a) was correct must precede the legitimate use of (a). For if this last demand were pressed, it would render deductive inference no less than inductive inference logically impossible. If we were never entitled to *use* a correct rule of inference before we had formally argued in support of that rule, the process of inference could never get started.

I shall end by considering an ingenious counter-example provided by Salmon. He asks us to consider the following argument:

(a'): In most instances of the use of R' in arguments with true premises in a wide variety of conditions, R' has been *un*successful.

Hence (*probably*)

In the next instance to be encountered of the use of R' in an argument with a true premise, R' will be successful.

The relevant rule is the "counter-inductive" one:

R': To argue from *Most instances of A's examined in a wide variety of conditions have not been B* to (probably) *The next A to be encountered will be B.*

Salmon says that while (a') must be regarded as a self-supporting argument by my criteria, the rule here supported, R', is in conflict

established by a self-supporting argument, according to Black" (p. 45)—if "established" means the same as "proved reliable." The self-supporting argument can *strengthen* the rule, and in this way "*support*" it.

with R. From the same premises the two rules "will almost always produce contrary conclusions."[10] This must be granted. But Salmon apparently overlooks an important respect in which the "counter-inductive" rule R' must be regarded as illegitimate.

In calling an inductive rule "correct," so that it meets the canons of legitimacy of *inductive* rules of inference, we claim at least that the rule is reliable, in the sense of usually leading from true premises to true conclusions. That is part of what we *mean* by a "correct inductive rule." It can easily be shown that R' must fail to meet this condition.

Suppose we were using R' to predict the terms of a series of 1's and 0's, of which the first three terms were known to be 1's. Then our first two predictions might be as follows (shown by underlining):

1 1 1 0 0

At this point, suppose R' has been used successfully in each of the two predictions, so that the series is in fact now observed to be 1 1 1 0 0. Since 1's still predominate, direct application of the rule calls for 0 to be predicted next. On the other hand, the second-order argument shows that R' has been successful each time and therefore demands that it not be trusted next time, i.e., calls for the prediction of 1. So the very definition of R' renders it impossible for the rule to be successful without being *incoherent*.[11] The suggested second-order argument in support of R' could be formulated only if R' were known to be unreliable, and would therefore be worthless. So we have an *a priori* reason for preferring R to its competitor R'. But it is easy to produce any number of alternative rules of inductive inference, none of which suffers from the fatal defect of R'. The choice between such rules, I suggest, has to be made in the light of experience of their use. I have tried to show in outline how such experience can properly be invoked without logical circularity.

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[10] Salmon, *loc. cit.*, p. 46.

[11] A parallel situation would arise in the use of R in predicting the members of the 1-0 series only if R were to be predominantly *un*successful. But then we would have the best of reasons for assigning R zero strength, and the second-order argument would be pointless.

WHAT IS A SCIENTIFIC NATURALIST AT MID-CENTURY?

I SHALL try in this paper to give an account of scientific naturalism as it appears to me to exist in the 1950's. Scientific naturalism better expresses what I want to describe than philosophical naturalism because I believe that "ultimate reliance on scientific method in . . . naturalism, as prior to any specific justification of naturalistic categories"[1] makes it semantically justifiable to call such contemporary naturalism "scientific."[2] At the same time it will be clear that this shows a certain arbitrariness on my part, for naturalism has different clusters of meaning at different periods in the history of science-philosophy.[3] "Naturalism" for Hegel simply meant "materialism,"[4] and the "naturalism" that Woodbridge called Aristotelian[5] is certainly not to be identified with literary or existentialist naturalism.

I am sure that naturalism can be ingeniously subdivided in many plausible ways. One can discuss it in terms of the presiding ideas of chosen periods in the history of science-philosophy, or in terms of national boundaries (French naturalism), or linguistic groups (Chinese naturalism). One may also speak of naturalism in terms of subject-matter, as in esthetic naturalism, ethical naturalism,[6] or psychological naturalism.

According to one historical point of view, " 'Naturalism' came into vogue as the name for a recognized philosophical position . . . which put man and his experience squarely into the Nature over against which he had hitherto been set."[7] This was a step made

[1] Abraham Edel, "Is Naturalism Arbitrary?," this JOURNAL, Vol. 43 (March 14, 1946), p. 147.

[2] The reader should be warned that the use of "scientific" by the naturalist does not imply "that all non-scientific concerns are for the naturalist . . . washed down the drain" (Carl Cohen, "Naturalism and the Method of *Verstehen*," this JOURNAL, Vol. 51 (April 1, 1954), p. 225).

[3] See Leonora Cohen Rosenfield's review of Vartanian's "Diderot and Descartes," this JOURNAL, Vol. 53 (August 30, 1956), pp. 558–559.

[4] Hegel, *Lectures on the History of Philosophy*, tr. by E. S. Haldane and F. H. Simson, Vol. III (New York, Humanities Press, 1955), p. 398.

[5] Woodbridge, *Nature and Mind* (New York, Columbia University Press, 1937); further developed by Sterling Lamprecht, *Our Philosophical Traditions* (New York, Appleton-Century-Crofts, 1955), pp. 77–78.

[6] See John Hospers, *An Introduction to Philosophical Analysis* (New York, Prentice-Hall, 1953), pp. 500 ff., for an account of esthetic naturalism, and pp. 455 ff. for ethical naturalism.

[7] John H. Randall, Jr., "Epilogue: The Nature of Naturalism," *Naturalism and the Human Spirit*, ed. by Y. H. Krikorian (New York, Columbia University Press, 1944), p. 356.

possible in the United States by the successes of nineteenth-century biology, and it is in the United States that naturalism has had its greatest recent triumphs. R. B. Perry in 1921 stated another perspective which remains a useful guide for determining certain directions of naturalism. He said, "By naturalism is meant the philosophical generalization of science—the application of the theories of science to the problems of philosophy." [8] By 1954 Perry had stated a broader view of naturalism which we shall have occasion to mention later.

Ernest Nagel has suggestively stated that "since naturalism is not a tightly integrated system of philosophy, perhaps the sole bond uniting all varieties of naturalists is that *temper of mind* [my italics] which seeks to understand the flux of events in terms of the behaviors of identifiable bodies." [9] Nagel's phrase, "temper of mind," like Paul Henle's notion that "naturalism is an attitude rather than a body of doctrine," [10] is helpfully suggestive. Having myself tried to use this scale, "temper of mind," in tracing naturalism in Indian philosophy, I can say that it is often a deceptive device in the sense that clues to a temper of mind usually turn out to be stated beliefs, unless we are able to live with the philosopher to observe his behavior and thus understand his temper somatically as well as linguistically. I know several theologians, for example, whose temper I believe to be "naturalistic," but I *feel* this about them by observing their everyday activities rather than by hearing their sermons.

I wish to use two major criteria to determine whether or not a philosopher is a scientific naturalist in contemporary America. The first criterion is temper of mind and the second is the actual doctrines believed and espoused by the philosopher in question. The two are really quite inseparable because (1) one cannot have an attitude without its having consequences in belief and (2) beliefs are clues to attitudes.[11]

I am now going to try to describe the scientific naturalist in terms of (1) what he must believe in order to be one, (2) what he

[8] Perry, *Present Philosophical Tendencies* (New York, Longmans, Green, 1921), p. 45. Lamprecht has given us some important insights into the development of naturalism, *op. cit.*, p. 180 and p. 244.

[9] Nagel, "Logic Without Ontology," *Readings in Philosophical Analysis*, ed. by H. Feigl and W. Sellars (New York, Appleton-Century-Crofts, 1949), p. 192.

[10] Personal communication, May, 1954.

[11] See Sol Levy's discussion, "On the Tautologous Nature of Stevenson's Distinction Between Disagreement in Belief and Disagreement in Attitude," this JOURNAL, Vol. 49 (March 13, 1952), pp. 177–191.

rejects if he is one, (3) some secondary opinions he may hold, (4) a thumbnail sketch of the scientific naturalist as a composite, and (5) a list of contemporary scientific-naturalistic schools.

I. What the Scientific Naturalist Believes

First, the scientific naturalist accepts empiricism as the most important avenue of knowledge, although not necessarily the only one.[12]

Second, the scientific naturalist believes that knowledge is public, that it can be discovered by ordinary people if they will take sufficient trouble to look for it. Knowledge is not esoteric nor given only to spiritually highly-gifted people who "know" things by inexplicable means not open to consensual[13] inspection.

Third, the scientific naturalist believes that the external world is objective in the sense that it is not "my idea," "a construct," *Aufbau* of appearance, entertained, postulated and/or hypostatized. Metaphysical agnosticism is rejected and "To know physical nature is to exceed the presented by the represented. . . ."[14] The scientific naturalist approves Woodbridge's "Confession" where he says, "And when I read, 'Knowledge is not eating and we can not be expected to devour what we mean,' I found the vanities of epistemology exposed."[15] This is an enormously crucial point, because here, as nowhere else, is it possible to separate the boys from the men, the idealists from the realists, materialists, and naturalists. The scientific naturalist at this point shows his sophistication in matters of scientific-philosophical history and parts company with the philosopher dominated by certain phases of acculturation making a bogyman of materialism, for example.[16]

Fourth, the naturalist takes metaphysics to be a meaningful and important discipline "continuous with scientific method . . . [in] an obstinate attempt to understand the ways of being of things

[12] See R. B. Perry, *Realms of Value* (Cambridge, Harvard University Press, 1954), p. 452.

[13] Borrowed from Harry Stack Sullivan's notion of "consensual validation." See his *The Interpersonal Theory of Psychiatry*, ed. by Helen S. Perry and Mary Ladd Gawel (New York, Norton, 1953), pp. 28–29.

[14] Perry, *op. cit.*, p. 453.

[15] Woodbridge, *Nature and Mind*, p. 4.

[16] According to Engels: "By the word materialism the philistine understands gluttony, drunkenness, lust of the eye, lust of the flesh, arrogance, cupidity, avarice, miserliness, profit-hunting and stock exchange swindling—in short, all the filthy vices in which he himself indulges in private" (*Ludwig Feuerbach*, New York, International Publishers, 1941, p. 31).

as a whole. . . ."[17] Another view of the acceptability of metaphysics is that of W. R. Dennes, who says, "If by 'metaphysics' we mean human science developed as far as possible, accompanied by . . . careful analysis of the categories and the methods employed in description and explanation and guided by serious judgment as to the directions of study most likely to safeguard and advance human values . . . then a critical naturalistic philosophy would not only admit metaphysics but would also contribute to its task of analysis."[18] This does not imply, however, that the scientific naturalist is without distrust of metaphysics-in-the-grand-manner, for naturalists since the time of Anaximander[19] have been profoundly suspicious of apocalyptic visions, thunderous absolutes, and unshakeable certainties, to paraphrase Ernest Nagel.[20]

Fifth, for the scientific naturalist, the external and also the internal world manifest order and regularity, which cannot be changed by entertainment, postulation, reconstruction of appearances, magic, sacrifice, or prayer. For this reason, the scientific naturalist is highly sympathetic not only to physics, chemistry, and biology, but also to the social sciences. A sure mark of the anti-naturalist is a manifestation of profound distrust of the social sciences and serious misgivings about quantification in them.

Sixth, the naturalist not only accepts the sciences and the scientific enterprise, but is enthusiastic about them. This is nowhere more zealously shown than by Woodbridge in his inaugural address at the University of Berlin in 1931. Woodbridge, who for some reason was not sufficiently terrified by the prospects of mechanism, said among many things like it: "The birds can be scientifically analysed. Still they sing."[21] Usually we find the other interpretation, that the important thing about birds is their "spiritual" qualities; here, however, Woodbridge implies that even if birds are mechanisms this does not detract from their abilities. The scientific naturalist may have greater optimism about what the new sciences will be able to do than some of the workers engaged directly in the enterprise. He does not drag his feet and bring charges characterizing science as cold, unfeeling, presumptuous, sterile, in-

[17] Patrick Romanell, quoted by J. H. Randall, Jr., in the latter's review of *Verso un naturalismo critico* [etc.], this JOURNAL, Vol. 51 (June 24, 1954), p. 390.

[18] Dennes, "The Categories of Naturalism," in Krikorian, *op. cit.*, p. 293.

[19] See W. I. Matson, "The Naturalism of Anaximander," *Review of Metaphysics*, Vol. 6 (March, 1953), pp. 387–395.

[20] Nagel, "Philosophy and the American Temper," in his *Sovereign Reason* (Glencoe, Ill., Free Press, 1954), p. 57.

[21] Woodbridge, *Nature and Mind*, p. 259.

human, bloodless, glassy, destructive of values, merely temporal, as if these could reveal internal weaknesses in the very structure of the scientific outlook.[22]

Seventh, supernatural teleology is given no credence by the scientific naturalist, nor does he believe in the occurrence of miraculous events, supernatural visitations, emanations, or the propitiation of supernatural beings. In short, "supernaturalism is rejected as an impossible assumption for intelligent and informed minds in the twentieth century. . . ."[23] John Dewey has made several classical statements concerning the naturalistic view of supernaturalism. In 1934 he said, "The objection to supernaturalism is that it stands in the way of an effective realization of the sweep and depth of the implications of natural human relations."[24] In 1944, in one of his clearest and most acid polemics, Dewey said, "Aside from displaying systematic disrespect for scientific method, supernaturalists deny the findings of science when the latter conflict with any of the dogmas of their creed. The story of the conflict of theology and science is the well-known result."[25]

Eighth, the scientific naturalist is humanistic in some sense of scientific humanism.[26] The welfare of the human as human concerns him most. Man is not simply a mirror of deity (a view which I call divine reductionism), but an individual in his own right. His goal is to do what is proper to man; it is impossible and absurd for man to "try to be like God," even if he could discover in all conscience what this phrase meant.

I believe that these are eight indispensable doctrines of scientific naturalism and are necessarily involved in what is called the naturalistic temper. Views such as that of the late revered J. B. Pratt should be excluded from scientific naturalism, particularly when he says, "no mystical state of the soul . . . no achievements of faith . . . no religious benediction . . . fails of its due appreciation or of its lofty position among the discriminated values of

[22] Sir Charles Sherrington has implied that the emotional reaction to the scientific "universe" as "lonely" for example may be the result of some imbalance. See *Man on His Nature* (New York, Doubleday Anchor, 1953), p. 282.

[23] Richard W. Boynton, *Beyond Mythology* (New York, Doubleday, 1952), p. 221. Boynton proves to be a master of gamesmanship in countering the hackneyed "spiritualism" of virulent antinaturalism.

[24] Dewey, *A Common Faith* (New Haven, Yale University Press, 1934), p. 80.

[25] Dewey, "Antinaturalism in Extremis," in Krikorian, *op. cit.*, p. 11.

[26] Not to be confused with Renaissance humanism, Christian humanism, Existentialist humanism, or Literary humanism.

critical naturalism"[27]; or his sketch of the "sincerely religious thinker" moving hand in hand with the naturalist toward a final revelation of *Deus Sive Natura*.[28] Pratt's concern with the vagaries of "crass" naturalism (as opposed to "elevated" naturalism), mechanism, and materialism indicates that he favors a view combining scientific naturalism with some of the ingratiating qualities of edifying non-naturalism.[29]

II. What the Naturalist Rejects

Another way of viewing scientific naturalism is from the standpoint of its rejections, which are logically implicit in its positive beliefs. The naturalist is opposed to (1) phenomenalism, phenomenology, neo-Kantianism, neo-Platonism, and other forms of mindism, (2) supernaturalism, (3) spiritualism in any usual sense of that term,[30] (4) mysticism, (5) authoritarianism based on supernaturalism or uncritical naturalism, (6) intuitionism except as "intuition" is used to indicate rapid association and deduction, and (7) anti-scientism and all forms of open hostility to or serious misgivings about the worth of the scientific enterprise.[31]

The scientific naturalist has a tendency to reject mindistic attitudes toward knowledge when these evince disembodied knowers, or regard knowing itself as something over and above the world of natural events.[32] For the naturalist, the "mind" is at most a biophysical-cultural-interpersonal sheath, envelope,[33] or what have

[27] Pratt, *Naturalism* (New Haven, Yale University Press, 1939), p. 163.

[28] *Ibid.*, p. 178.

[29] Those critics, but surely not Pratt, who are "well content to pass their lives with their bellies to the table and their eyes looking up to heaven . . ." (Llewelyn Powys, *Damnable Opinions*, London, Watts, 1935, p. 113).

[30] Susan L. Stebbing has given us one definition of "spiritual" which a scientific naturalist could accept. She says, "The excellences I call *spiritual* include love for human beings, delight in creative activities of all kinds, respect for truth, satisfaction in learning . . ." (*Ideals and Illusions*, London, Watts, 1948, pp. 29–30).

[31] The anti-scientists include not only technical philosophers, but other fashionable prophets of gloom, including the Eliot Circle of Anglo-Catholics, the Neo-Orthodoxers, Neo-Thomists, and the Wobbling Scientists who have lately begun to worry about the general problem of human welfare and in their understandable haste have mistaken antinaturalist soap for philosophical cream, including several eminent Nobel Prize winners, more generally in the physical sciences. The biologists, for reasons put forward by Leuba some years ago, are less susceptible to knight errantry.

[32] Roy Wood Sellars, *The Philosophy of Physical Realism* (New York, Macmillan, 1932), pp. 433–435.

[33] A term used by Harry Stack Sullivan to avoid dualistic accounts of mind.

you, but not "superior" to the brain in some special spiritualistic-mindistic-occultist-symbolistic way. He does not believe that the mind can be extended into the past and future, be non-spatial, and possess meanings wholly absent from the "mere" brain.[34]

To many philosophers, the supernatural speaks of the poetic, the holy, and the emotionally necessary, but to the naturalist talk of the supernatural is not about something objective or even probably existing. His attitude toward the supernatural reveals also his attitude toward spiritualism and mysticism. The spiritual, when it is defined or described in such a way as to imply "other" [35] universes and "other" natures, is rejected by him, although the spiritual as signifying the natural aspirations of man is perfectly acceptable.[36] The "other" world views are always conveniently vague and tenuous so that they are not open to knowledge and hence rational criticism. The one tacit syllogism of all supernaturalism, sufficiently advanced to be aware of the serious challenge of scientific naturalism, is the following:

> Naturalism cannot explain everything.
> But everything *must* be explained.
> Therefore: Naturalism is inadequate.[37]

Mysticism, since it generally depends upon the recognition of, proximity to, or intercourse with, something supernatural, is also rejected, although a blanket rejection of it has a strange consequence when we examine the nature-mysticism of some forms of Taoism and Buddhism. D. T. Suzuki, a leading Zen Buddhist, has recently shown a marked tendency to reaffirm a nature-mysticism, developed in his 1953 paper read at Ascona, Switzerland, entitled, "The Role of Nature in Zen Buddhism." Suzuki said, "From the human point of view, anything that it not of human origin may be said to be of Nature. But Man is, after all, part of Nature itself. First of all, Man himself is not Man-made, but Nature-made, as much as anything we regard as of Nature. If so, what is Man-made? There is nothing in Man that does not belong in Nature." [38]

Authoritarianism, which requires that utterances be considered

[34] Sellars, *ibid.* See also Krikorian, *op. cit.*, "A Naturalistic View of Mind."

[35] See Iredell Jenkins, "Naturalism," in D. Runes, *The Dictionary of Philosophy* (New York, Philosophical Library, 1942).

[36] Boynton, *op. cit.*, *e.g.*, "the spiritual is the valuable, and the valuable is the spiritual" (p. 225).

[37] Chapman Cohen, *Materialism Re-Stated* (London, Pioneer Press, 1927), p. 45.

[38] Suzuki, reprinted in *Studies in Zen* (New York, Philosophical Library, 1955), p. 178.

true simply because they are made by certain super-enlightened individuals, or received from holy literature, is given no allegiance by naturalists nor even by many mindists these days.

III. Some Secondary, but not Necessary Beliefs of Scientific Naturalism

Among the secondary doctrines of scientific naturalism, I wish to include the following: (1) a marked curiosity about the operations of the physico-psycho-socio-world of events; (2) a belief that abstract ideas are not objective and external (or internal in a conceptualistic sense), but constructions of the "mind"—horses exist independent of minds, but horse-ness does not; (3) a third view is that empiricism is not only the most important avenue of knowledge, but the only reliable one (with logic tacitly accepted, of course); (4) a belief that the "body" and "soul" are not separate entities, but "fused together" in an indissoluble latterday Lucretian unity; and finally (5) a belief that the "physical" side of man should be given as much stress as the satisfactions we might call mental or spiritual as the last is used by Stebbing or Boynton. If there is no strong approbation of hedonism among scientific naturalists, there is at least a subdued attack upon it. The naturalist does not overlook or underplay "physical" satisfactions nor show great disapproval of tangible rather than so-called intangible goods.

IV. A Note on the So-Called Naturalistic Temper

What I shall say now may cause some repugnance, anguish, pain, or even merriment among scientific naturalists who find flaws in the informal characterology in which I wish to indulge. This brief excursion into philosophical personality description follows a lead suggested by Rollo Handy in a recent issue of *Philosophy of Science*,[39] but he is in no way responsible for the statements made.

The scientific naturalist is generally a mesomorph in body-type (following the lead of Sheldon and Morris), is not sparing in his praise of physical satisfactions such as the opportune enjoyment of wines, cuisine, and love. This recalls to mind a phrase of Anatole France that "Christianity has done much for love by making a sin of it."[40] Can we say that sometimes the scientific naturalist takes the *sins* of the antinaturalists and makes virtues of them?

It is perhaps not unfair to say that the scientific naturalist is

[39] Handy, "Personality Factors and Intellectual Production," Vol. 23, No. 4 (October, 1956), pp. 325–332.

[40] France, *The Garden of Epicurus*, tr. by Alfred Allinson (London, Lane, 1920), p. 17.

earthy, irreverent, and common in the sense that he is less likely to mistake ideas for things than his more symbolically oriented brothers. He likes contact with the physical world and with people who are "down to earth," and becomes impatient with pious effusions sometimes mistakenly called "statements of high ideals." The scientific naturalist is less disturbed by the "grosser" and "more sordid" activities of the people in low-income brackets, less concerned also by the increase in gadgets, automation, and mechanistic applications. Finally, the scientific naturalist avoids much pseudologic and autism,[41] is less rigid, more at home in the world, more humane and less picayunish about recognized "animal qualities" in himself and therefore does not take kindly to the pious self-mesmerization with which antinaturalists build up their security-systems. The traditional caricature of the philosopher, interestingly enough, is of a man who is other-worldly, autistic, rising above inferior, if not outright disgusting, nature and the brawling naturalists who do not condemn the "materialistic outlook."

V. Scientific Naturalistic Schools of Thought Today

I wish, finally, to simply list some of the naturalisic schools of thought today and give in parenthesis one leading exponent of each view. Some of these schools may be fading from the scene, others changing their names, but despite this flux I should like to designate the following as schools, groups, or pockets of scientific naturalism.

The California Circle (Dennes)
Contextual Naturalism (Nagel)
Critical Materialism (Farber)
Cybernetic Materialism (Culbertson)
Dialectical Materialism (Selsam)
Experimentalism (Churchman)
Instrumentalism (Dewey)
Naturalistic Humanism (Woodbridge)
The New York Circle (Krikorian)
Physical Futurism (Seelye)
Reformed Materialism (Sellars)

There are doubtless other groups of which I am ignorant, but whose existence will be contemplated with pleasure by the scientific naturalists belonging to the schools above.

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[41] See G. K. Zipf, *Human Behavior and the Principle of Least Effort* (Cambridge, Mass., Addison-Wesley, 1949), pp. 284–286.

THE ANTINOMY OF INDIVIDUALS

RECENT discussions of the principle of the identity of indiscernibles have brought to light certain counter-examples, which seem to show that this principle is false.[1] Professor Ayer has suggested that the formation of these examples, such as that of the symmetrical universe, involves a use of the distinction between subjects and predicates which allows them to take for granted what the principle intends to deny.[2] The purpose of this paper is to discuss an antinomy which appears to result from the sort of distinctions upon which these counter-examples seem to rest. It is not our purpose to argue that these examples do rest on any particular distinctions, but rather to argue that *if* they do rest on the sort of distinctions which we shall specify, then an antinomy concerning individuals arises.

This antinomy may be approached by considering two questions which are complementary.

(1) Is it logically possible that two individuals may be absolutely different, i.e., different in *all* respects, and the same in *no* respects?

(2) Is it logically possible that two individuals may be absolutely the same, i.e., the same in *all* respects, and different in *no* respects?

Let us suppose that the above two questions presuppose that subjects are distinct from predicates in the following manner. Let us suppose that they imply that individuals are denoted by a certain sub-class of logical subjects, namely, indexical terms, such as "this," "it," "Socrates," and that predicates alone describe the respects in which individuals may differ or be the same. Thus, given certain restrictions concerning what is admitted as a descriptive predicate, or on what can specify a "qualitative difference," these individuals cannot differ or be the same as denotata of indexical terms taken in abstraction from the applicable predicates, but can only differ or be the same in respect of whatever predicates apply to them. Accordingly, the query concerning logical possibility which each question makes seems to be implicitly answered in the affirmative by the very terms in which the questions are asked. If the distinction between individuals as the denotata of indexical terms is logically prior to their distinction *qua* predicates applicable to them, the logical limits of difference and sameness

[1] Cf. Max Black, "The Identity of Indiscernibles," *Mind*, Vol. LXI (1952), pp. 153–164; N. L. Wilson, "The Identity of Indiscernibles and the Symmetrical Universe," *Mind*, Vol. LXII (1953), pp. 506–511.

[2] A. J. Ayer, "The Identity of Indiscernibles," in *Philosophical Essays*, p. 34.

seem to be absolute difference and absolute sameness. There does not seem to be any logical reason why either or both of these limits should be impossible. Sameness and difference, then, pertain only to applicable predicates. It is to be understood that "sameness" and "difference" are themselves predicates. Whether they are conceived to be simple, one-place predicates or more plausibly to be two-place or three-place predicates does not matter as long as it is agreed that they are in some sense predicational terms.

We shall call individuals conceived of as denotata of indexical terms, and in abstraction from all applicable predicates, "abstract individuals." In view of the foregoing, questions (1) and (2) seem to be puzzling in their implications. Even though they seem to imply a distinction between abstract individuals prior to distinction in respect of applicable predicates, *taken together*, they also seem to imply that abstract individuals may differ and be the same. In other words, even after we make a distinction between subjects and predicates in the above manner, our use of the singular word "individual" and the plural word "individuals" suggests that we can apply the predicational terms "same" and "different" to subjects as such. And if this is implied in our talk about individuals, an antinomy arises. For the two questions complement one another in that (1) seems to imply that two individuals might *differ* in all respects, except as denotata of indexical terms, in which, however, they are still in some sense *individual*, i.e., the same as abstract individuals. And (2) seems to imply that two individuals might be the *same* in all respects, except as denotata of indexical terms, in which, nevertheless, they are still in some sense *different* abstract individuals. The first question *ex hypothesi* guarantees the predicational diversity of individuals and pushes this diversity to its logical limit, excluding all predicational sameness. And if this is admitted as a logical possibility, the question arises as to what meaning the term "individual" (or any other term which serves the same use) could possibly have in such a case, if abstract individuals cannot in any sense be the same. If these two radically different individuals are in *no* sense the same, in what sense are they both individuals? It might be argued that they are in no predicational sense the same, and must be "shown" rather than described. But the difficulty with this is that (a) we do speak of them as "individuals," and (b) we apparently cannot speak of what can only be shown. If individuals are thus ineffable, it does not seem to be possible even to say that they can be shown or pointed to, or for that matter even that they are ineffable. In short, unless we admit that abstract individuals are in some sense the same, it will be sheer self-deception to talk about

a link between language and "the world." The world-end of each link will dangle helplessly in the mists of the unutterable. On the other hand, if we *do* talk about individuals, and we admit the logical possibilities of the above questions, there is a strong suggestion that we are tacitly assuming that abstract individuals may differ and be the same.

The second question *ex hypothesi* guarantees the sameness of individuals and pushes this sameness to its logical limit, excluding all predicational difference. And if this is admitted as a logical possibility, the familiar question arises as to what meaning the plurality of "individuals" could possibly have in such a case. This question suggests, in effect, that the plurality of abstract individuals may be as ineffable as the individual itself in the case of the possibility of question (1).

So far, we have said nothing about what we mean by sameness and difference of abstract individuals. It might be objected to our procedure that we are illegitimately re-introducing the notion of predicational sameness and difference after we have explicitly excluded it from the notion of an abstract individual. There is no difficulty in meeting this objection, as the point of it may readily be granted. In fact, we wish to emphasize it. We maintain that if individuals are not ineffable, and their plurality is not ineffable, and a distinction between abstract individuals and their applicable predicates is made, then any talk about individuals will, in view of the logical possibilities suggested by questions (1) and (2), covertly re-introduce the notions of sameness and difference of abstract individuals.

In speaking of the sameness and difference of abstract individuals, we have deliberately refrained from making a distinction between "difference" and "numerical distinctness" or "numerical diversity," for we maintain that in the present context no such distinction is appropriate. The notion of an abstract individual, as we have defined it, seems to be logically simple. We have said that abstract individuals are *in some sense* the same, and *in some sense* different. But if the notion of an abstract individual is logically simple, the senses in which they are the same and different, whether they are "numerically" the same and different, or the same and different in some other way, cannot be distinguished. Thus, if we talk about individuals, and imply a radical distinction between subjects and predicates, we seem to be faced with a puzzling antinomy. In the light of the above logical possibilities, abstract individuals are both the same and different in a unitary sense. But if it be insisted that in view of the logical possibility of question (2), individuals may be numerically distinct but not different with

respect to applicable predicates, the antinomy can be formulated in these terms just as well. If question (2) implies that abstract individuals may be numerically distinct but not different, question (1) implies that they may be numerically non-distinct but not the same. Hence, abstract individuals are both numerically distinct and numerically non-distinct. Over against the possibility of the radically symmetrical universe, we must place the possibility of the radically asymmetrical universe. If the numerical distinctness and non-difference of individuals is logically possible as in a radically symmetrical universe, then it would seem that the numerical non-distinctness and difference of individuals is equally possible as in a radically asymmetrical universe. If it be argued that the latter is self-contradictory, it can just as well be argued that the former is self-contradictory. If it be said that we cannot imagine the latter universe, it can likewise be said the we cannot imagine the former. If it requires some special insight, perhaps akin to Spinoza's *scientia intuitiva*, to see how two different individuals can yet be one, no doubt it also requires a special insight to see how two individuals which are absolutely the same can yet be numerically distinct.

If the second question is self-contradictory, then the principle of the identity of indiscernibles, viz., that if we have two individuals, then they differ with respect to at least one applicable predicate, would seem to be necessarily true. And if the first question is self-contradictory, then it would seem that another principle, which might be called the principle of the diversity of discernibles, viz., that if we have two individuals, then they must be the same with respect to at least one applicable predicate, is necessarily true. It will be observed that if (1) and (2) are interpreted as implying a contradiction, the contradiction in each case is implied in the expression "two individuals." In each case, the qualification specified can be construed as rendering duality logically impossible. Each principle intends to set up certain conditions under which individuals are possible, or conditions under which use of the term "individual" is meaningful. Whereas the principle of the identity of indiscernibles intends to rule out diversity of individuals where there is no discernible difference, the principle of the diversity of discernibles intends to rule out application of the term "individual" where there is no discernible sameness. Both principles seem to rest on equally plausible philosophic insights. The former seems to arise, as Professor Ayer remarks, from the doubt ". . . whether there can be any sense in talking of a plurality of objects unless it is a way of talking about difference of properties." [3]

[3] A. J. Ayer, *op. cit.*, p. 28.

And the latter would seem to stem from the doubt that there can be any sense in employing a single term, such as "individual," unless it is a way of talking about sameness of properties. The former expresses the view that many individuals must be discernibly *many*, and the latter expresses the view that they must be discernibly *individual*. If we believe that both principles are true, we should require that the manyness of things should have a descriptive meaning, and that the thingness of things should have a descriptive meaning. Whether either one or both of these requirements can be met is, indeed, open to question. It seems clear, however, that to distinguish between subjects and predicates and also to talk about subjects in abstraction from their applicable predicates is to involve oneself in an antinomy.

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BOOK REVIEWS

Man and People. JOSÉ ORTEGA Y GASSET. Authorized Translation from the Spanish by Willard R. Trask. New York: W. W. Norton & Co. [1957]. 272 p. $4.50.

José Ortega y Gasset's *Man and People* is, in my opinion, a seminal prologomenon to any future sociology, not only on the strength of its wealth of diversified insight, but because it offers an analysis of the "social," clearly setting it off from the realms of the individual and the inter-individual. This is philosophical sociology, a refreshing change from the pretentious jargon of social science as well as from the Christian anthropologies of academic evangelists. While the representatives of the former almost invariably neglect to examine the limitations of the method to which their findings necessarily conform, the latter depend on certain assumptions or categories such as original sin which (we are told) man is not self-sufficient enough to question. On the one hand, the result is pseudo-science with its familiar pattern of illegitimate objectification and dehumanization; on the other, "edifying discourses" which often contain a great deal of value though begging the question of the grounds from which it is derived.

Ortega dissociates himself from the social scientists inasmuch as even the most eminent among them, he tells us, fail to define the object of their inquiries—society; he takes exception to the existentialists for misusing the word exist (for to live), and he regards the positivists as sophomoric. Since he is not theologically oriented in his thinking, his position is, broadly speaking, humanistic or

Romantic with strong leanings towards phenomenology. His method is Socratic, his *finesse* Pascalian, and his outlook Jamesian—open minded. He remains the individual even as thinker.

What concerns him above all is the *Lebenswelt*—not the world which physics reveals to us (differently every century or so), not, *in itself*, the world of plants, animals, and vegetables devoid of "reciprocal inwardness," but the world in which Man since time immemorial has had to come to grips with circumstance in general and, most conspicuously, with the Other, fellow-man, in particular. (For Ortega modern science is but another form of human action, no less an interpretation of reality than a poem, and the latter is no less a way of knowledge than the former.) From Hume, Kant, Hegel, and the existentialists Ortega develops the distinction between a world of things subsisting in their absoluteness and a world of things existing *for us*, that is to say, giving us pleasure or displeasure, perplexing or resisting us, being adverse or serviceable depending on our *Blick*—the situation in which we happen to find ourselves with respect to them. For each of us this situation is constantly changing and for no two of us is it the same at any one time. Thus the starting point of Ortega's sociology becomes the "radical solitude" of the individual. Nobody else can live our lives for us, make our decisions, enter into our inwardness. We are alive before we are scientists because things exist for us as interests and concerns prior to being viewed as abstractions. Minding or loving is a much more primordial act than proving.

In addition to being personal, untransferable, and circumstantial, human life is perilous. Again and again Ortega stresses the peculiar danger to which we are exposed, especially when we feel ourselves most secure. By "peril" here he means the tenuous nature of being human in contrast to the stable nature of being a tiger, for example—the ever-present temptation of appropriating forms of human life that are non-human, like the collectivist who denies the dimension of the personal in his life. Man on this view is unendowed with a nature that he can take for granted. His attributes have developed over thousands of years of civilization, and they are constantly threatened from within as from without. (Femininity, for Ortega, is essentially biographical rather than biological, a work of art rather than a natural fact.) Freedom for the individual resides in the choice he has among those possibilities that define his destiny. But he cannot help doing or preferring something. To do "anything" is to espouse nihilism. The dialectic of being at once human and non-human defines the uniqueness of human being. Only man can relapse into barbarism and remain himself; only man can acquire saintliness and remain finite. Enter-

ing into politics with the passion with which Socrates entered into himself constitutes for Ortega the *daemon* of our times, the most tempting variety of dehumanization.

Beyond Aristotle's world of things, which doesn't account for Truth, and Descartes's world of ideas, which doesn't account for the world we live in, Ortega makes human life his focal point. What we know with maximum clarity and distinctness, for instance, is a pain of our own. In fact, we know it so well, he suggests, that we would not trade it for something more remotely connected with us. (The opposite of pleasure is *not* pain, for many pains are pleasant.)

Ortega's emphasis on the fundamental reality of human life as radical solitude should not be misconstrued as solipsism, since the horizon in which man has his being includes animals, plants, minerals; furthermore, "when we first become aware that we are living, we already find ourselves not only with others and among others, but accustomed to others" (p. 106). Our relationship to our fellow human beings is of course infinitely more complex than our relationship to nature. As a matter of fact, for Ortega the original encounter between Adam and Eve was nothing less than traumatic. How much more we must fear our neighbor than the dog down the road or the lightning in the sky! As St. Augustine remarked in his *Confessions*, even family happiness is the most precarious of treasures. Whether man in relation to fellow-man will be social or dissocial, friendly or hostile, rational or irrational is always an unpredictable event. Human life is perilous if for no other reason than that we are as prone to mistrust as to love.

It is the encounter of the individual with the Other that enables him to recognize himself as an ego. But, Ortega is careful to point out, inter-individual relationships must be clearly differentiated from what is society. Whereas the one implies a transformation of the Other into the You, the other remains impersonal, without soul or spirit, to whatever degree it may abstractly compel our behavior. "The social appears not, as has hitherto been believed and as was far too obvious, when we oppose it to the individual, but when we contrast it with the inter-individual" (p. 179). Relationships between individuals, if they develop positively, are always marked by trust, reciprocity, and mutual responsibility. The Other becomes the You through an overcoming of mutual fear, hostility, and alienation. Each partner of a friendship is intellectually and morally committed to respecting the other. Conversely, hatred between enemies is will-engendered and self-originating. There is nothing anonymous or impersonal about the forces which make two people dislike each other intensely.

Why, asks Ortega, do I shake hands when I join a gathering?

Why do I allow the traffic-policeman to keep me from crossing the street? Why do I submit to the laws of a State—a Kafkaesque abstraction, a hidden force with which I cannot enter into any concrete relationship? (Incidentally, Ortega makes much of the *compresence* of things in our line of vision. From apples to societies we are determined to see just a bit of them at a time.) Society, in contrast to inter-individual relationships, is made up of "indeterminate" men and "impersonal" usages that are best symbolized by the pronoun, "one." One obeys the law, one dresses for dinner in a certain way, in Rome one does as the Romans do, etc., etc., but one doesn't fall in love with one's wife, one doesn't die, one doesn't decide to become a Christian. One doesn't live! Only human beings live, and already that is an abstraction. Men and women—rather, this man and that woman, John, Mary, Dick—they alone are truly real, they alone love, die, and become Christians. Society, in a nutshell, rests on a "gigantic architecture of usages" which are slowly established and slow to die out, to which we are anonymously driven to conform, and which, for the most part, are laxly enforced. It is in this context that Ortega offers a brilliant interpretation of the salutation in human experience. The bellicose salutation recently in vogue in fascist states, he remarks, is not a genuine salutation because it does not wish health to the individual; it is here one day and gone the next, and its sanctions are so closely tied up with violence that it lacks the amorphous, incomprehensible force of an established social usage.

In the spirit of Kierkegaard, Ortega bemoans the idolatry of society that is a characteristic feature of modern thought and life: "The collective soul, *Volksgeist* or 'national spirit,' social consciousness, has had the loftiest and most marvelous qualities attributed to it, sometimes even divine qualities. For Durkheim, society is veritably God. In the Catholic De Bonald (the actual inventor of collectivistic thought), in the Protestant Hegel, in the materialist Karl Marx, this collective soul appears as something infinitely above, infinitely more human than man. For example, it is wiser. And here our analysis, with no special effort or premeditation, with no formal precedents (at least so far as I am aware) among philosophers, drops into our hand something disquieting and even terrible—namely, the *collectivity* is indeed something human, but the human without man, the human without spirit, the human without soul, the human dehumanized." (Pp. 174–175.)

Man and People also includes an essay on "stylistics" stressing the inseparability of the concrete word and the situation in which it is uttered, and an exhilarating critique of Mademoiselle de Beauvoir's *Second Sex*—"to believe as follows from her argument, that

a woman is more a person when she does not 'exist' preoccupied by man but occupied in writing a book on 'the second sex,' seems to us something decidedly more than confusion" (p. 132). Willard R. Trask's translation from the Spanish reads smoothly, more smoothly than American sociologists in their native tongue.

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Gnosis und spätantiker Geist. HANS JONAS. Zweiter Teil, erste Hälfte: *Von der Mythologie zur mystischen Philosophie.* Göttingen: Vandenhoeck & Ruprecht, 1954. xv, 223 p. (Forschungen zur Religion und Literatur des Alten und Neuen Testaments, hrsg. von D. Rud. Bultmann. Neue Folge, 45. Heft. Der ganzen Reihe 63. Heft.) DM 18.00.

The first volume of this work was originally published in 1934 and has now been republished together with the volume here under consideration. To this second volume the present review will limit itself.

Gnosticism presents an extraordinary challenge to a historian of ideas. The first reaction to gnostic texts (of which a sample will be found further on) is likely to be one of unbelief that anybody in his right mind could have dreamt up all these myths. It is even harder to believe that they were taken seriously by a considerable number of people. The great majority of literature on gnosticism is not very helpful. It is essentially doxographic and does not even try to interpret. It also studies the relations between Christianity and gnosticism, between gnosticism and Greek philosophy, the ultimate sources of gnosticism, etc., but as a rule is satisfied with comparing and contrasting the external appearance of ideas, so to speak, hardly ever asking what they actually mean. Jonas' book is of an entirely different order. It attempts to understand.

The fundamental categories of understanding Jonas takes over from Heidegger's *Sein und Zeit.* This may sound prohibitive to some inclined to consider Heidegger so obscure a writer that the application of his ideas could only confuse the issues. Let it therefore be said that Jonas handles these categories so adroitly that some may find him a good guide towards the understanding not only of gnosis but at the same time also of Heidegger.[1]

What are these categories? They all concern the nature of man.

[1] As is well known, in his attempt to "demythologize" Christianity Bultmann also makes use of Heidegger's categories. Despite my respect for Bultmann the theologian, I submit that, though Jonas is his pupil, he has a much surer grasp of philosophy than has his master.

Man is a specimen of being which is constituted by its self-understanding. The fundamental trait of this self-understanding is that man constitutes himself as being "of" the cosmos or, as we could also say, as a mundane being. This does not mean that man is *in* the cosmos as if he and the cosmos were two objects somehow related to each other, e.g., spatially, causally, by the relation of knowing and being known, etc. It is, on the contrary, man's mundaneity which makes all these kinds of encountering the cosmos possible. The mundaneity (*In-der-Welt-sein*) is the quality of being man. But instead of realizing this, man conceives of the cosmos as of an object which he faces. He now exists "in" a cosmos, forgetting that he is his own mundaneity. And now he begins to derive what he considers primary categories of his self-understanding from this cosmos—as if Cervantes should ask Don Quijote to tell him who Cervantes is, forgetting that he is Cervantes because he is the author of *Don Quijote*, that therefore Don Quijote is a symptomatic revelation of what Cervantes is, but can give no answers. Sometimes also the opposite takes place and man tries to understand the cosmos in human categories, allegedly derived from his self-understanding. He anthropomorphizes the world. But in so doing he overlooks that before that he has already reified himself by interpreting himself as being *in* the cosmos. At the basis of all relations between man and the cosmos is therefore the dark and threatening feeling that something absolutely essential has been forgotten which, if brought to light, would simply annihilate everything.

Perhaps I may add that this mood of dread over having forgotten something has been expressed with incomparable force in a story by Gogol. A boy is refused the hand of a girl he loves because he is poor. He is desperate, when suddenly the devil appears and promises him riches if only he will murder a certain child whom he loves very much. The boy does it—and in the same moment finds himself in possession of riches. But he has also completely forgotten how he came by them. He marries the girl and lives with her, permanently tormented by the feeling that he has forgotten something which he must remember but cannot.

In the case of gnosis (but see below) Jonas calls this mood of dread (*Angst*) cosmic dread (*Weltangst*). Man feels that there is some terrible mistake in his being in this cosmos which seems to threaten and oppress him. He is not aware that this mood of dread is the fundamental mood of his being accompanying his mundaneity. Instead, he explains his cosmic dread as the result of some quality of the cosmos itself and of himself being in it. And he proceeds to invent a story (myth) which would explain this quality and his

dread. The myth tells him that this cosmos is an evil thing, something anti-divine and separated by an abyss from the absolutely transcendental god and his realm of light and goodness. Moreover, the myth tells him that he also contains a spark of the divine (the pneuma) in himself, which spark (in him and others) should be delivered from the cosmos (decosmized, *entweltlicht*) and led back to its source. The myth explains how all this happened in terms of "stories" (beginning with the creation of "spirits" by the transcendental god, one of which spirits in some way "falls") and also explains man's place in the scheme of things. Finally, the myth describes the return of the spark (in the form of a journey), to take place after man's death. The hearing and understanding (gnosis) of this myth (the illumination) is the first step towards decosmization, and this the myth proclaims as part of its own contents (the myth is its own object). The second step is the participation in some rites (mysteries, magic actions) which will provide the gnostic with "passwords" securing him safe conduct on his journey back to the transcendent god.

Gnosticism therefore offers only a preparation for deliverance from the cosmos. The actual deliverance will take place only after death. Precisely here the gnostic myth starts developing into mystical philosophy, the essence of which is the promise that this deliverance can be in some way anticipated already in this life. In the ecstatic *unio mystica* the cosmos can be extinguished. But before mystic philosophy can fully develop, the gnostic myth on which it is based must first be profoundly altered and the whole gnosis "rationalized." This is effected by transforming the persons ("spirits") who had appeared in the myth as actors into impersonal emanations, and by replacing the arbitrary actions of these persons (or, at best, personifications) by some kind of logical order which determines the necessary order of these emanations. The dualism of the evil cosmos and the transcendent god is transformed into monism, in which the cosmos is no longer god's adversary but simply his other. It is this transformation which permits the mystic to experience as an internal pilgrimage what the gnostic myth had described as an external (*post mortem*) journey—a pilgrimage on which the soul in this life already reascends by the same steps (emanations) by which it descended and does not have to pass by some sentinels, guards, etc., whose will it must influence. Myth has become mysticism, gnosis has turned into the philosophic system of Plotinus.

This is the main line of Jonas' interpretation. Of collateral topics two deserve special mention. One concerns the difference between Christianity and gnosticism. In Christianity the impossi-

bility of anticipating deliverance already in this life is conceived as an essential quality of human life; therefore a Christian does not attempt such an anticipation. The other concerns Philo. With great acumen Jonas disentangles all the irreconcilable strands in Philo's theology. Sometimes Philo speaks as if man's supreme exertion of cognitive powers could result in some kind of adequate knowledge of God, and this knowledge is sometimes described as ecstasy, sometimes as merely the highest degree of intellectual insight. Sometimes, however, this exertion is supposed to lead to the knowledge of God's "that" but never of his "what." Sometimes it is merely the exertion itself, regardless of the fact that it must fail, which Philo describes as the maximum which man can and should achieve.

All interpretations of Jonas are keen and interesting. Not all are convincing. The main objections, some historic, some systematic, follow.

1. Gnosis is supposed to answer man's cosmic dread. Jonas never makes it clear whether he considers this *Weltangst* (others may prefer the translation "cosmic anxiety" or "cosmic anguish") to be a fundamental trait of man or of some men only. If he assumes the former, how does he explain that we are not all gnostics? If he assumes the latter, how does he explain its presence in some, its absence in others?

2. That the concept of a *unio mystica* presupposes the depersonalization of the actors of the gnostic myth is doubtful. There seem to be many mystics who conceive of their *unio mystica* as uniting them with a personal god.

3. The monism, another presupposition by which mythology turns into mysticism, implies according to Jonas that the achievement of the *unio mystica* is not only a change in the condition of the mystic (decosmizing him) but also in that of the deity with whom he enters into union. Now, some mystics will see it this way. Angelus Silesius said, "I know that God cannot live without me even for a moment—When I die He is necessitated to give up His ghost" (and Rilke repeated after him his "What will you, God, do when I die? I am your garment—what if I am rent?—I am your vessel—what if I be broken?," etc.). But would all mystics think so? Jonas quotes no texts to support his thesis. I doubt whether he could find any in Plotinus. It is on the contrary a formula of Porphyry that God is always near man, but man not always near God. One can be a monist without assuming that the relation between God and man is reciprocally symmetrical.

4. Having accepted Heidegger's categories, Jonas must assume that all of man's knowledge-of . . . (his knowledge of himself or of

the cosmos) is actually man's mode of being and thus not at all what it pretends to be. To accept this pretension means to falsify one's mode of being—something like trying to place the responsibility for one's shape on one's shadow. Gnostic knowledge cannot be an exception to this. Jonas indicates repeatedly that his interpretation of gnosis must eventually lead to its destruction (just as Heidegger designates as his goal the destruction of all ontology, i.e., of all understanding of being). Such a destruction would open the road towards authentic existence, i.e., one in which knowledge-of . . . is understood as a symptom and nct as a doctrine with a specific (true or false) content. But these indications are never followed up.

5. Jonas is very sure that the interpretation of the universe as a *system* of emanations and therefore of philosophy as its mirror and thus a system itself was unknown to Greek thinkers before the 3rd century A.D. One wonders whether this is said by Jonas with full knowledge of what Aristotle presented (e.g., in his *Metaphysics, A*) as the gist of Plato's philosophy.

Much becomes clear through the interpretation of Jonas. But many dark corners remain. Here is part of a gnostic myth (of the Manichean variety). To prepare deliverance a good male spirit shows to his twelve male adversaries twelve nude female deities. This so excites the adversaries that they have an emission of sperm, which falls on the earth and here generates offspring. Now the male spirit shows himself in the nude to his twelve female adversaries, who all are permanently pregnant. The sight so excites them that they give birth. The fetuses also fall on the earth, eat some of the offspring of the sperm, and thereupon start copulating.

What kind of mind must he have who feels that the hearing of this kind of myth will have liberating effects on him![2] There are limits in our ability to understand our fellow-men and here they seem to be reached. But Jonas seems to be unaware of any such limits.

All these doubts and objections notwithstanding, it is a high pleasure to read Jonas' book, whether one is interested primarily in systematic or in historic problems.

Bibliographical Note

Gnosticism is at home in the borderland between philosophy and religion. It is therefore appropriate, perhaps, to give the philoso-

[2] Let us hope that future generations won't experience the same shock of "is this possible!" which such a myth is likely to administer to us, on discovering that in the 20th century the theories of Freud and Jung were formulated and widely accepted.

pher some bibliographical hints. A good (doxographic) introduction is H. Leisegang, *Die Gnosis* (Leipzig, 1924); to Manicheism, the article "Manichäismus," by H. J. Polotsky, in the Pauly-Wissowa *Realencyclopädie*. On gnosticism in the Hermetic literature, particularly its relations to Greek philosophy, see A. J. Festugière, *La Révélation d'Hermès Trismégiste*, 4 vols. (Paris, 1944–54). Important new finds of gnostic manuscripts have recently been made. They are not yet generally accessible, but from the reports of G. Quispel (e.g., in his *Gnosis als Weltreligion*, Zürich, 1951) it seems to follow that they will enlarge rather than change our picture of gnosticism (to the interpretation of which Quispel applies categories of Jung, a homoeopathic treatment indeed). For a recent survey of the theories of Harnack, Bousset, Reitzenstein, etc., on the historic filiations of gnosticism see W. Foerster, "Das Wesen der Gnosis," *Die Welt als Geschichte*, Vol. 15 (1955), pp. 100–114.

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